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Do Parental Networks Pay Off? Linking Children's Labor-Market Outcomes to Their Parents' Friends*

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Abstract

In this paper, we examine whether children are better off if their parents have more elaborate social networks. Using data on high-school friendships of parents, we analyze whether the number and characteristics of friends affect the labor-market outcomes of children. While parental friendships formed in high school appear long lasting, we find no significant impact on their children's occupational choices and earnings prospects. These results do not change when we account for network endogeneity, network persistency, and network measurement error. Only when children enter the labor market do friends of parents have a marginally significant but small influence on their occupational choice.

Keywords: Informal job search; intergenerational effects; occupational choice; social networks

JEL classification: *A*14; *J*24; *J*46; *J*62

I. Introduction

Social networks are widely considered important for labor-market outcomes (Jackson, 2010). In search models, social networks are typically thought of as informal job-search channels providing job searchers with either information about open vacancies or background references,

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recommendations and job referrals (Rees, 1966; Granovetter, 1973). Furthermore, in surveys, social networks are often mentioned as one of the main channels through which job searchers find jobs (Holzer, 1987, 1988; Ioannides and Loury, 2004; Cappellari and Tatsiramos, 2015).

However, quantifying social networks and their impact on labor-market success has proved difficult. First, social networks are often loosely defined and can take many shapes and forms, ranging from family members and friends to colleagues, dormmates, neighbors, and ethnic-minority groups.[1] Second, information on social networks is rarely collected together with information on labor-market outcomes. Third, causal inference is difficult due to the potential endogeneity of network connections (Manski, 1993; Bramoullé *et al*., 2009).

In this paper, we are the first to examine whether children are better off if their parents have more elaborate social networks. Specifically, we focus on the high-school friendships of parents and test whether the number and characteristics of high-school friends affect the labor-market outcomes of children. Our empirical strategy takes into account some of the selectivity effects that are common to studies on the labor-market consequences of social networks. In particular, we examine how sensitive our results are to network measurement error, network persistency, and network endogeneity.

We use data from the Wisconsin Longitudinal Study (WLS). The WLS contains detailed information on a random sample of Wisconsin high-school graduates in 1957 (Hauser, 2009 provides an overview of the WLS structure). Respondents are asked about their friendship connections in high school, which can be used to reconstruct the underlying friendship network. Respondents also report their children's occupational choice, which we measure in terms of prospective earnings and interpret as a proxy for lifetime earnings. We exploit the richness of the WLS, including information on the respondents' cognitive and non-cognitive abilities, educational attainment and other socioeconomic variables, to account for many of the individual characteristics that possibly confound friendship ties. The WLS information on friendship ties has been used before by Conti *et al*. (2013).

We start our empirical analysis by examining whether children, parents, and high-school friends of parents make similar occupational choices. We do not find evidence for the presence of friendship-network effects. We find positive correlations between the occupations of children and the friends of their parents, but these positive correlations disappear as soon as we account for coinciding occupational choices between parents and children.

[1] Examples are Kramarz and Nordström Skans (2014), Cappellari and Tatsiramos (2015), Cingano and Rosolia (2012), Marmaros and Sacerdote (2006), Topa (2001), and Edin *et al*. (2003).

We next analyze the relationship between the number and characteristics of friends and the labor-market outcomes of children. Again, we find that the quantity and quality of friendship ties do not influence the occupational choices and earnings prospects of children, with the exception of a small significant effect shortly after children entered the labor market.

Our paper relates to a few recent papers that focus on the impact of social networks on labor-market outcomes within an intergenerational context. Kramarz and Nordström Skans (2014) analyze the relevance of family and classroom networks for the school-to-work transition of high-school graduates in Sweden. Using matched employer–employee data taken from administrative registers, they look at how children's own parents as well as the parents of the children's high-school classmates affect the likelihood of the children working at similar firms. They find that children are significantly more likely to start working at firms that also employ their parents, but not at firms that employ their classmates' parents. These family-network effects are most pronounced for low-educated children. Olivetti *et al*. (2013) analyze the impact of family and friendship networks on female labor supply (measured at the intensive margin). Using intergenerational information taken from the Add Health dataset, they estimate the extent to which the labor supply of women depends on the labor supply of their mothers and that of their friends' mothers. They find that women work more if they, as teenagers, had working mothers as well as friends with working mothers. These family and friendship-network effects are equally strong. Both papers focus on network ties between children and their parents, and between children and their classmates and friends. In contrast, we focus on network ties between children, parents, and their parents' high-school friends. Therefore, the implications are different. If, for example, old-boys networks are important in determining the labor-market outcomes of children, we expect that networks based on parents and their high-school friends are more suited to pick this up than networks based on children and their friends' parents.

Our paper also contributes to a larger body of literature in economics on the intergenerational effects of economic outcomes. In the context of labor-market outcomes, there are many empirical studies that report strong and positive associations between earnings and occupational choices of parents and their children (Lentz and Laband, 1989; Solon, 1992; Björklund and Jäntti, 1997). In recent years, a growing number of studies have put more emphasis on causal intergenerational effects, reporting substantially smaller parental effect estimates, thus revealing the importance of heritability and other selection effects (Behrman and Rosenzweig, 2002; Plug, 2004; Holmlund *et al*., 2011).

The remainder of this paper is organized as follows. In Section II, we describe the data. We define measures for the size and quality of a

friendship network and we discuss the earnings score as a labor-market outcome. In Section III, we present the estimation results. In Section IV, we conduct several robustness tests to account for network endogeneity, network persistency, and network measurement error. Finally, we conclude in Section V.

II. Data and Descriptive Statistics

The WLS provides detailed survey data on 10,317 individuals who graduated from high school in 1957, which constitutes a random one-third sample of all graduates in Wisconsin in that year (e.g., Hauser, 2009; Sewell *et al*., 2003).[2] Individuals have been interviewed during six waves (1957, 1964, 1975, 1992, 2004, and 2011) to collect detailed information on education, labor-market outcomes, and measures of cognitive and non-cognitive skills. In 1975, 18 years after high-school graduation, individuals were asked to list their high-school friends. In later waves, respondents were also asked about basic characteristics and some labor-market outcomes of one of their (randomly selected) children. We use information on the 6,481 children included in the 2004 wave.[3] Table 1 provides summary statistics for the main variables we use in our analysis.

Occupations and Earnings Scores

We focus on the primary occupations of respondents and their children. Occupations of parents (i.e., respondents) are measured in 1992 when they are between 52 and 55 years old. For the children, we use occupations reported in both the 1992 and 2004 survey. Occupations observed in 1992 are those at the beginning of a working career when the children are, on average, 26 years old. However, some children have not completed their education at that time and thus are not observed. Furthermore, the reported occupation might be less representative for individual employment histories, as young workers are more likely to change occupations again at a later stage. Therefore, we mainly focus our analysis on occupations observed in 2004 when the age of their children ranges from 28 to 50

[2] We thank the National Institute on Aging (AG-9775), the National Science Foundation (SBR-9320660), the Spencer Foundation, and the Center for Demography and Ecology and the Vilas Estate Trust at the University of Wisconsin-Madison for their support in collecting and disseminating data from the WLS. Only we bear the responsibility for the further analysis or interpretation of these data. Data and documentation from the WLS are available at http://dpls.dacc.wisc.edu/WLS/wlsearch.htm.

[3] Reasons for the difference between the initial number of respondents and the number of children in the 2004 survey include childlessness, usual sample attrition, and in some cases refusal to answer the WLS questionnaires.

Table 1. *Descriptive statistics*

	Mean	Standard deviation	Observations
Child outcomes			
Earnings score 1992	27.87	19.71	5,373
Earnings score 2004	35.94	20.37	5,969
Child characteristics			
Female	0.48	0.50	6,481
Age (in 2004)	38.05	4.12	6,479
Parent characteristics			
Female	0.54	0.50	6,481
Age (in 1992)	53.14	0.49	6,480
Earnings score 1992	32.27	20.40	6,024
Years of college	1.81	2.66	6,288
IQ score	101.64	14.47	6,481
Extraversion score	3.91	1.03	6,273
Agreeableness score	3.87	0.95	6,274
Conscientiousness score	3.12	1.09	6,273
Neuroticism score	4.86	0.78	6,271
Openness score	4.87	0.76	6,271

years, with an average of 38 years. Previous studies have shown that current income within this range proxies lifetime income most accurately for the US (e.g., Haider and Solon, 2006).

In the WLS, occupational choices of respondents and their children are coded in line with the definitions of the US census in 1990. We use two classification schemes in our analysis. The first classification summarizes occupations into 18 distinct categories. Corresponding frequency distributions for both respondents and their children can be found in the Online Appendix. The second classification summarizes occupations into 501 distinct categories. In the latter case, the WLS provides various measures of occupational prestige, such as educational requirements, and average earning prospects. We focus on the occupational earnings score, which indicates the fraction of workers in a given occupation earning at least US$14.30 per hour in 1989 according to 1990 US census data.[4] A comparison between the respondents' annual earnings (defined as the sum of wages, salaries, commissions, and tips before taxes and other deductions) and earnings scores in 1992 shows that both measures are

[4] Hauser and Warren (1997) provide a review of measures of occupational status, including various occupational earnings scores. One measures the percentage of workers in an occupation who earn more than US$25,000 in 1989. Another measures the percentage of workers in an occupation who earn US$14.30 per hour or more in 1989. This wage rate corresponds to earnings of US$25,000 per year for workers who work 35 hours per week and 50 weeks per year.

strongly correlated.[5] Thus, the occupational earnings score can be regarded as a good proxy for labor-income prospects.

Using earnings scores has several advantages in the analysis of occupational choices. First, it provides a continuous measure of the average returns to occupational choices. As the earnings score is the same for all workers in a given occupation, the measure abstracts from earning differences due to individual heterogeneity and quantifies the potential payoff independently of worker-specific skills. This reduces the threat of biased estimates because of correlations between unobserved ability and earnings. Second, and more importantly, the earnings score can be interpreted as a proxy for lifetime earnings. Occupational choices are evaluated not only in terms of current payoffs, but also with respect to the average earnings across all workers in the US census. Interpreting the score as a measure of lifetime earnings implicitly assumes that the occupation does not change considerably during the life cycle with respect to prospective earnings. A comparison between reported occupations in 1992 and 2004 shows that the earnings scores vary only modestly, with correlation coefficients of 0.72 and 0.49 for parents and children, respectively.

As shown in Table 1, the earnings score averages are 27.9 and 35.9 percentage points for children in 1992 and 2004, and 32.3 percentage points for parents in 1992. As expected, earnings scores are, on average, considerably lower for children at the start of their occupational career. A difference-in-means test between the 1992 score of parents and the 2004 score of children also confirms that the younger generation works in occupations with significantly higher earnings scores ($p < 0.0001$), suggesting intergenerational differences in occupational choices.[6] To get a better idea of the distribution of earnings scores, Figure 1 plots the cumulative distribution for children and parents. It shows that earnings scores vary between the 4th and the 88th percentile and are relatively equally distributed apart from a slightly concave shape at higher percentiles. Compared with actual annual earnings of WLS respondents, the distribution of earnings scores is by construction smoother and has no outliers.

Friendship Measures

In 1975, respondents are asked to list the three best same-sex friends from their high-school senior class. The WLS contains information about

[5] The correlation between workers' annual earnings and earnings score in 1992 is 0.46 with a *p*-value of less than 0.001. As actual earnings are not reported for children of respondents, we cannot compute the same correlation for this generation.

[6] Comparing earnings scores between children and parents of the same wave, in 1992 or 2004, leads to similar results.

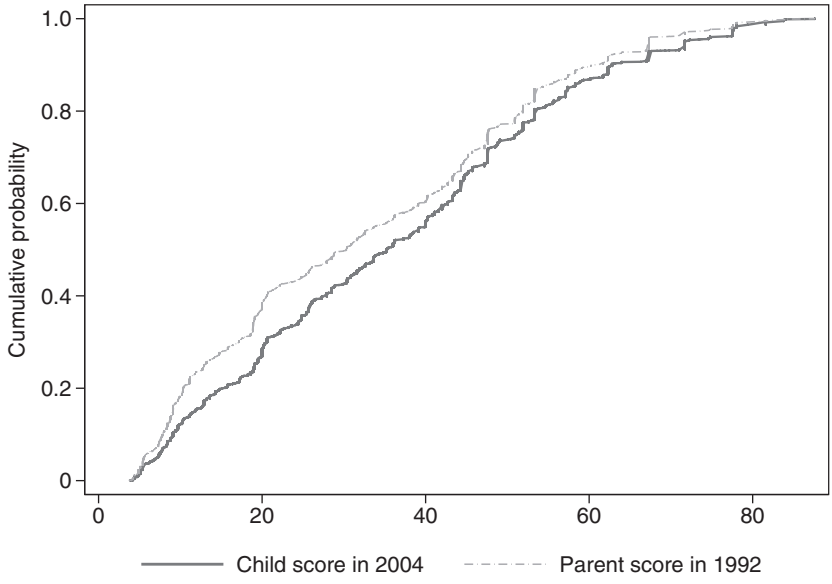

Fig. 1. Occupational earnings score distribution

the number of claims that can be matched to other high-school graduates in the cohort.[7] Some of the claims are matched to other high-school graduates in the WLS, which allows us to reconstruct substantial parts of the friendship network in high school.[8] As the WLS sample represents a one-third share of all Wisconsin high-school graduates in 1957, survey data on characteristics of friends are available for approximately this fraction of friendship claims. According to previous research, US students form the majority of friendships within high school (Ennett and Bauman, 1996). Thus, the claims should capture the respondents' friends in 1957 reasonably well.

For each respondent in the WLS, we observe friendship links that are nominated by the individual (outgoing connections), as well as links with the individual that are nominated by other respondents (incoming connections). Borrowing the terminology of graph theory, we call the number of outgoing connections the out-degree, and the number of incoming connections the in-degree. Furthermore, we observe whether

[7] In some cases, this number deviates from the number initially reported if respondents cannot remember their friend's full name, misspell the name, or claim by mistake friends outside the cohort.

[8] Conti *et al*. (2013) use this feature of the WLS friendship data to study the impact of popularity on labor-market outcomes.

connections are reciprocal and nominated by both sides (reciprocated connections). These friendship connections are arguably stronger and more persistent than non-reciprocated connections and can be used to measure network effects for two different strengths of friendship ties. We refer to reciprocated connections as strong connections.[9] Next, we construct a measure that takes all connections of a respondent in high school into account (total friendship connections). It is defined as the sum of the out-degree and in-degree connections corrected for double counting of the reciprocated friendship connections.

These friendship measures are subject to systematic measurement error. In particular, the observed in-degrees are incomplete because the WLS data cover only one-third of all potential high-school friends. Whether a respondent is claimed as a friend is observed only for connections who are interviewed by the WLS. As a result, complete coverage of reciprocal friends and total friendship connections are not available. To illustrate this, Figure 2 depicts an example of a high-school graduate who claims three friends and is also claimed as a friend by three other individuals. In this case, the in-degree is not fully observed because some friendship connections are outside the WLS. Furthermore, we do not observe for all claimed friends whether they are reciprocal.

Given that respondents with high in-degrees are more likely to have unobserved claims, missing observations introduce non-classical measurement error to the size of the network, which might lead to biased regression estimates. To correct the friendship measure for this error, we impute the expected number of received friendship claims based on the observed distribution and selection probability for each potential claim. As respondents can claim only same-sex friends, the imputation is performed separately for the number of female and male friends (see the Appendix).[10]

Table 2 provides summary statistics on the number of connections (network size) for each of the four friendship measures in the top panel. As shown in the first row, respondents claim, on average, 2.25 friends with a standard deviation of almost one friend. However, less than half of these claims are actually reciprocated. Contrary to that, the average number of received friends (in-degree) is similar to the out-degree, but shows a higher variation, as the number of claims is not restricted to three friends in this case. The last row summarizes the distribution of

[9] Similarly, social-network theory distinguishes between weak and strong connections to qualify interpersonal ties. According to the weak tie hypothesis initiated by Granovetter (1973), weaker connections are more relevant for the impact of social networks because individuals outside the direct social environment can also be reached. However, other studies argue that strong ties are of prior importance because more interaction takes place and more information is transmitted among these connections (e.g., Krackhardt, 1992).

[10] The imputation procedure is not applicable to the characteristics of female and male friends.

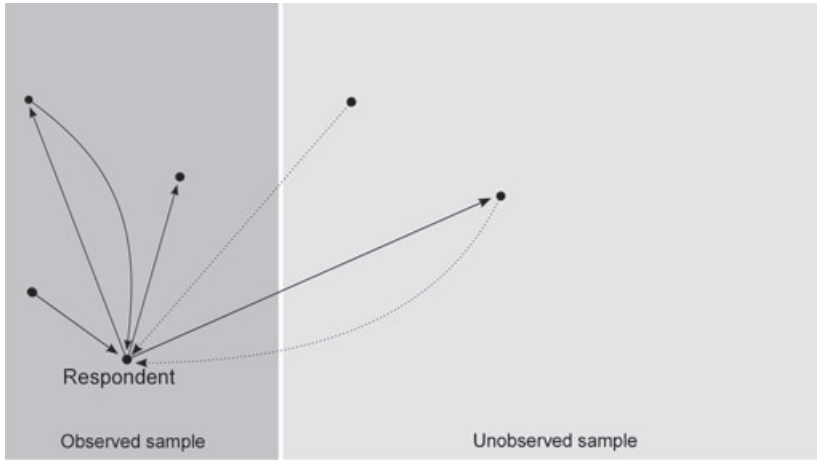

Fig. 2. Friendship ties in the network of Wisconsin high-school graduates

total connections, showing that an average individual is connected to 3.27 high-school friends. Furthermore, we observe that the average number of connections differs with respect to gender. According to all four friendship measures, female respondents have more connections than males.[11]

Table 2 also provides summary statistics for the average earnings score in all friendship categories in the bottom panel. As social contacts with high earnings scores might be better able to assist children in finding equally well-paid jobs, this variable can be regarded as a proxy for the quality of the network. For each of the four friendship measures, we compute the average earnings score across all observed connections.[12] Here systematic measurement error is less of a concern. Although not all high-school graduates are interviewed and information on earnings scores is available only for some friends, the friendship data are missing at random, conditional on the number of connections, because respondents

[11] It is possible to compare the number of out-degree friendships in Table 2 with other estimates of out-degree friendships. In the National Longitudinal Study of Adolescent Health (Add Health), for example, high-school students are asked to nominate five (and not three) same-sex high-school friends. Fletcher *et al*. (2013) report average numbers of nominated friends of 2.65 for male students and 3.10 for female students. Although their numbers of out-degree friendship connections are (only a little) larger than ours, they are similar in that male students nominate fewer friends than female students.

[12] We have experimented with alternative friendship quality measures such as the maximum earnings score of friends. Our friendship quality results appear to be insensitive to the quality measures we use (see the Online Appendix for regression results using the maximum earnings score of friends as the explaining variable).

Table 2. *The quantity and quality of friendship connections*

	Full sample			Female			Male		
	Mean	SD	*N*	Mean	SD	*N*	Mean	SD	*N*
Number of friends									
Out-degree	2.25	0.94	6,191	2.37	0.87	3,356	2.11	0.99	2835
Reciprocated	1.08	0.54	6,191	1.20	0.52	3,356	0.95	0.53	2,835
In-degree	2.10	1.27	6,191	2.23	1.18	3,356	1.95	1.34	2,835
Total connections	3.27	1.34	6,191	3.40	1.22	3,356	3.12	1.46	2,835
Earnings score of friends									
Out-degree	32.72	19.81	2,859	24.75	17.25	1,604	42.90	18.14	1,255
Reciprocated	32.04	19.87	1,466	25.35	17.68	893	42.46	18.58	573
In-degree	31.43	18.80	2,613	24.15	16.34	1,476	40.87	17.54	1,137
Total connections	32.09	18.79	3,716	24.09	16.10	2,043	41.85	17.15	1,673

Notes: The number of reciprocated, in-degree, and total friendship connections are corrected for measurement error.

are selected randomly. This means that our measure of friendship quality is an unbiased measure of network quality.[13]

Parental Characteristics

The WLS contains information on cognitive and non-cognitive skills of the respondents. Cognitive skills are measured in the 1957 wave by means of the Henmon–Nelson test of mental ability. The test score results are converted to standard IQ scores. Non-cognitive skills are assessed in the 1992 wave, together with information on the respondents' labor-market careers, based on the Big Five Inventory developed by John *et al*. (1991). Five personality traits (openness, conscientiousness, extraversion, agreeableness, and neuroticism) are taken from five to seven questionnaire items for each trait, where the magnitude of these item attributes are measured on a one-to-six scale. Using this information, we calculate average scores for each personality trait. To avoid imprecise measurement, scores are coded as missing if respondents answer fewer than two items per attribute. According to the five-factor model, the combination of these traits provides a proficient summary of individual personality (Goldberg, 1990; Costa and McCrae, 1992). In our analysis, we think of these cognitive and

[13] There is another issue of sample selection; that is, respondents with more friends are over-represented because characteristics of friends are less likely missing. Of course, missing earnings scores could also be imputed based on available data. This requires additional assumptions on the earnings score distribution across friends. If we assume linear dependence between the earnings scores of a respondent's friends, we can impute values for all friendship claims and test the sensitivity of our network quality results. We find that our results do not change in any meaningful way.

non-cognitive skill variables as fixed when parents form their friendships (Grönqvist *et al*., 2017 report that in Swedish data the correlation between cognitive skills measured at age 13 and 18 is 0.87).

III. Empirical Analysis

Occupational Choice

Table 3 reports the observed matches in main occupations between children and their parents and between children and the high-school friendship connections of their parents. Matches refer to those children who work in the same occupation as their parents and as their parents' friends. Occupations are based on the 18 main occupation categories.[14] When focusing on the friendship connections of parents, for each child, we divide the number of matches by the number of friendship connections. The reported shares represent averages across all individuals. To test for associations between occupational choices, we compare the observed matching rates with those that would result from random matching. Assuming that occupational choices are random draws from the empirical distributions of children, parents, and parents' friends, we calculate for each subgroup the expected share of random matches.

Table 3 provides results for matches with occupations of children measured in both 1992 and 2004. Using early occupational choices of children (upper panel), we find that in about 16 percent of all cases, the occupations of parents and children match. This is significantly different from the 11 percent matches that would occur if parents and children would randomly choose their occupations. The observed matching share with the parents' friends of 13 percent is considerably lower, but still significantly different from the random matching share, regardless of the type of friendship connections.

One explanation for the observed matches with parents' friends might be that occupational choices of the friends are correlated with those of the parents, and thus simply proxy the direct intergenerational link. To account for this possibility, we additionally calculate the matching shares between children and friends for the subsample of children who do not work in the same occupation as their parents. We find that the matching share falls to 11 percent, which resembles the random matching share. This suggests that children are significantly more likely to end up working in occupations in which their parents work, but not in occupations in which

[14] An analysis based on the more detailed occupation codes leads by construction to very few matches, which makes a reliable evaluation difficult. In the next subsection, we return to the detailed occupational codes.

Table 3. *Observed and random matches with main occupations of children*

Match with	Observed match	Random match	*p*-value
Major occupation of children in 1992			
Parent	0.156	0.114	0.000
Total connections of parent	0.132	0.116	0.002
– if parent's occupation different	0.111	0.109	0.372
Major occupation of children in 2004			
Parent	0.173	0.120	0.000
Total connections of parent	0.138	0.124	0.004
– if parent's occupation different	0.119	0.115	0.211

Notes: The *p*-value corresponds to a one-sided *t*-test of the hypothesis that observed matching rates exceed random matching rates.

their parents' friends work once the occupation of parents is taken into account.

When we use occupations of children in 2004 (lower panel of Table 3), we find that matching shares are larger in all three cases. However, random matches are also more likely here. As for the early earnings scores, the *t*-tests indicate a significant association between major occupations of children and their parent's friends, which disappears if we focus only on children that do not work in the same occupation as their parents.[15]

Earnings Score

While children do not choose the same occupations as their parents' friends (once we account for the occupational choices of parents), it does not mean that parents' friends do not have any influence on the labor-market outcomes of children. The parents' friends might, for instance, help or motivate children to get into better-paid occupations other than their own. To examine such a potential payoff of friendship connections, we estimate a linear relationship between the prospective earnings of children and the friendship network of parents of the following form

$$Y_i^c = \alpha + \beta FN_i + \delta X_i^c + \gamma X_i^p + u_i, \tag{1}$$

where Y_i^c is the earnings score of a child in family i and FN_i is the friendship-network measure of the parent. Our parameter of interest is β, which captures the network effect on the child's earnings score. We

[15] In the Online Appendix, we also report occupational matching shares for mothers, fathers, sons, daughters, and combinations thereof. For all parent–child combinations, we find that the observed similarity in occupational choices among children and their parent's friends is (almost fully) driven by the observed similarity in occupational choices among children and their parents.

estimate the model using ordinary least squares (OLS). To give a causal interpretation to β, the friendship network should be independent of the error term u_i, conditional on the observed characteristics of the child and the parent (X_i^c and X_i^p, respectively). The observed characteristics should thus include variables that are related to the formation of a friendship network, which are probably characteristics other than just the basic characteristics such as gender and age. In the estimation, we use varying sets of observed characteristics including the cognitive and non-cognitive skill measures of parents.

As the network measure FN_i, we consider both the quantity and the quality of the friendship network of the parent. For network quantity, we use the total number of connections, which is defined as the sum of out-degree and in-degree friends corrected for double counting of reciprocal connections.[16] For network quality, we use the average earnings scores of the parents' friends. To show how observed characteristics affect the impact of friendship ties, we consecutively extend the set of control variables in the regression equation. For each friendship measure, the sample is restricted to individuals for whom information on the full set of characteristics is available. Furthermore, we perform the analysis separately for female and male respondents, to account for potential gender differences.

The earnings score of children is measured in 1992 and in 2004. While we use both earnings measures in our estimations, we consider the results for the 2004 earnings score more valuable. With the 1992 earnings score measure, we estimate network effects on censored samples of children who just entered the labor market when earnings differences are not that pronounced (yet). With the 2004 earnings score measure, we estimate network effects on uncensored samples of children who are in their peak earnings years when earnings represent lifetime earnings.

Number of Friends (Size of the Network)

Table 4 reports the impact on the earnings score of children in 2004 for six different specifications, where we use the total number of friendship connections as a measure for the size of the network. In Column 1, we show the marginal friendship effect in a model without other covariates. The coefficient is significantly different from zero and indicates that one additional friendship connection of the parent is associated with an earnings score increase of the child of 0.534 percentage points. However, the estimated association is very small given an earnings-score

[16] All estimates are based on the corrected friendship measures. Marginal effects for the (uncorrected) observed number of connections are summarized in the Online Appendix.

Table 4. *Marginal network-size effects on the child's earnings score*

	(1)	(2)	(3)	(4)	(5)	(6)
Friendship connections	0.534***	0.536***	0.585***	0.372*	0.307	0.214
	(0.204)	(0.200)	(0.201)	(0.200)	(0.199)	(0.198)
Child controls						
Female		−7.959***	−7.836***	−7.959***	−7.953***	−7.967***
		(0.547)	(0.544)	(0.540)	(0.536)	(0.531)
Age (in 2004)		4.463***	4.509***	4.251***	4.056***	3.851***
		(1.124)	(1.120)	(1.110)	(1.102)	(1.093)
Age squared		−0.062***	−0.062***	−0.058***	−0.055***	−0.051***
		(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Parent controls						
Female			−0.0629	−0.391	1.889***	1.887***
			(0.579)	(0.575)	(0.627)	(0.621)
Extraversion score			0.353	0.627**	0.608**	0.614**
			(0.281)	(0.280)	(0.278)	(0.276)
Agreeableness score			−1.138***	−0.755*	−0.623	−0.536
			(0.390)	(0.388)	(0.386)	(0.383)
Conscientiousness score			0.412	0.519	0.480	0.500
			(0.376)	(0.373)	(0.370)	(0.367)
Neuroticism score			−0.331	−0.0300	0.0516	0.0640
			(0.277)	(0.276)	(0.274)	(0.272)
Openness score			1.777***	1.126***	0.691**	0.178
			(0.311)	(0.315)	(0.316)	(0.318)
IQ score				0.195***	0.151***	0.092***
				(0.020)	(0.020)	(0.021)
Earnings score 1992					0.137***	0.096***
					(0.016)	(0.016)
Years of education						1.124***
						(0.118)
Intercept	34.12***	−41.62**	−46.80**	−64.03***	−60.99***	−52.20**
	(0.729)	(20.89)	(21.02)	(20.90)	(20.75)	(20.60)
Observations	5,290	5,290	5,290	5,290	5,290	5,290

Notes: The dependent variable is the child's earnings score measured in 2004. The independent variable of interest is the total number of friendship connections measured in 1992. Regressions contain varying sets of controls. Standard errors are in parentheses. ***, ** , and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

standard deviation of approximately 20 percentage points. In Columns 2–5, we add characteristics to the regression model that are arguably exogenous, including the child's gender and age, and measures of parents' cognitive and non-cognitive skills. In Column 2, we find that adding child characteristics does not alter the estimated network coefficient. The estimates for gender and age are nonetheless statistically significant and

similar to those found in most other wage regressions; that is, the earnings score is lower for women and concave in age. In Column 3, we also find that including personality traits does not change the friendship effect. Of the five personality traits, only agreeableness and openness to experiences affect the child's earnings score in a statistically significant way.[17] In Column 4, we add parental IQ and find that the total number of friendship connections continues to have a small but marginally significant effect on the child's earnings score. Parental IQ itself has a significantly positive impact, which suggests that high-IQ parents have, on average, more high-school friends, as well as more children who are more successful in the labor market.

In Columns 5 and 6, we also control for the earnings score and years of education of parents. However, including these parental characteristics as control variables in the earnings-score regressions can be called into question. In case parents' friends help parents to find jobs in higher-paying occupations, or influence their educational qualifications that enable parents to work in higher-paying occupations, the parents' educational attainment and earnings scores are outcome variables rather than control variables. Nonetheless, if we control for the parents' earnings score and years of education, we find that the estimated network coefficient does not change much. The impact of parents' friends on the child's earnings score is still insignificantly small, holding parental education, occupational earnings score, and other characteristics constant. As such, these findings coincide with those from the previous subsection, where the friendship connections of parents had no effect anymore after conditioning on parental outcomes.

So far, our analysis has focused on the earnings score of children in 2004, when most children are about 38 years old and likely work in their primary lifetime occupation. However, it is possible that friendship networks of parents are stronger at earlier stages of the child's occupational career. Job-market entrants might benefit more from social networks of their parents because they are less well connected themselves and less informed about employment prospects than older workers. Furthermore, employers are less able to evaluate the productivity of young workers and, thus, rely more often on informal referrals (see Hensvik and Nordström Skans, 2016). Alternatively, children might spend more time with their parents at young ages and better benefit from their friendship network.

To detect whether network effects are stronger in entry-level occupations, we repeat our analysis using the earnings score of children measured in

[17] In the WLS, Müller and Plug (2006) observe comparable personality patterns for earnings. Among fathers, they find positive returns for non-agreeableness and openness to experiences. Among mothers, they find positive returns for contentiousness and openness to experiences.

Table 5. *Marginal network-size effects*

	(1)	(2)	(3)	(4)
Full sample				
Child earnings score 1992	0.595***	0.571***	0.495**	0.417**
(N = 4,909)	(0.205)	(0.194)	(0.195)	(0.194)
Child earnings score 2004	0.534***	0.536***	0.372*	0.214
(N = 5,290)	(0.204)	(0.200)	(0.200)	(0.198)
Mothers				
Child earnings score 1992	0.543*	0.772***	0.636**	0.592**
(N = 2,643)	(0.310)	(0.295)	(0.295)	(0.294)
Child earnings score 2004	0.762**	0.812***	0.574*	0.457
(N = 2,791)	(0.313)	(0.307)	(0.305)	(0.302)
Fathers				
Child earnings score 1992	0.579**	0.478*	0.402	0.291
(N = 2,266)	(0.277)	(0.261)	(0.261)	(0.258)
Child earnings score 2004	0.428	0.374	0.218	0.031
(N = 2,499)	(0.272)	(0.267)	(0.267)	(0.261)
Child characteristics		✓	✓	✓
Parent characteristics			✓	✓
Parent outcomes				✓

Notes: The dependent variable is the child's earnings score measured in 1992 or 2004. The independent variable is the number of friends. Each estimate involves OLS regressions with varying sets of controls. Child controls include gender, age, and age squared. Parental controls include gender, five personality traits, and IQ test scores. Parental outcomes include earnings score and years of schooling. Standard errors are in parentheses. ***, ** , and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

1992. At this early stage, most children are about 26 years old, just finished their education, and started working in their first occupation.[18] The upper panel of Table 5 reports the network-effect estimates for entry-level occupations using the earnings score of children in 1992 as the outcome variable. We find that the number of friends has a stronger impact 12 years earlier. The estimates are also less sensitive to the inclusion of parent covariates, leading to consistently higher and statistically significant effects. In the richest specification, we find that a one-standard-deviation increase in the number of connections raises the earnings score of children by approximately 0.559 percentage points. However, compared with the overall variation in earnings scores, the network effects in entry-level occupations are at most modest.

[18] It is possible that some of the children in our sample have not finished their university education yet and report to work in a part-time or student jobs. However, the WLS occupations are reported only if children have worked at least six months in the same occupation.

Next, we split up the sample by gender of parent to test whether the mother's network has a different influence on their children than the father's network. We expect to see differences for a number of reasons. First, respondents of the WLS are asked to report same-sex friends; that is, we observe only the male friends for fathers and female friends for mothers. Second, simple network averages already show that mothers have a larger network than fathers. Third, previous studies report different intergenerational correlations for mothers and fathers (see the review by Haveman and Wolfe, 1995). When we run our network regressions on samples of mothers and fathers separately, we find that the small but positive friendship effects on the earnings score of children are mostly driven by the network of mothers. Table 5 shows that the network effects of mothers are all positive but get smaller when covariates are added. When we include the full set of covariates, we find that maternal network effects on child earnings scores measured in 2004 are insignificant. The impact on early occupations again remains significant, but the estimated coefficient of 0.592 earnings-score points is still small when compared with the overall variation in earnings scores. The network effects of fathers are, in most specifications, smaller than the network effects of mothers, and not statistically significant. When we use the children's early score in 2004 as the outcome, the impact of the father's friends is almost zero in the richest specification.

Furthermore, we test whether network effects are different for daughters and sons. As shown in the Online Appendix, the estimated coefficients are very similar. If we split the sample by both gender of parent and gender of child, we find that connections of mothers have a much larger impact on their sons, suggesting that the network effect is fully driven by this subsample. On the contrary, the number of father's friends has a stronger effect for daughters.

To see whether network effects are driven by a particular friendship channel, we decompose the total number of friends into non-reciprocated out-degree, non-reciprocated in-degree and reciprocated connections.[19] With these three friendship connection measures, we can estimate a more general specification of the earnings-score equation. Table 6 presents regression estimates using non-reciprocated out-degree, non-reciprocated in-degree, and reciprocated friendship connections as the right-hand-side variables. We find that non-reciprocated claimed friendships (out-degree) have a stronger association with the child's earnings score than

[19] Note that total in-degree, total out-degree and reciprocated connections do not add up to the total number friends. Therefore, we subtract reciprocated connections from in- and out-degrees for the regression. In the Online Appendix, we also report results for regressions where the separate friendship channels are included in isolation.

Table 6. *Total friendship effect by channel*

	Total connections	
	1992	2004
All	0.417**	0.214
	(0.194)	(0.198)
– non-reciprocated out-degree	0.709	0.756
	(0.514)	(0.525)
– non-reciprocated in-degree	0.424*	0.383
	(0.234)	(0.239)
– reciprocated	0.195	−0.669
	(0.552)	(0.564)
Observations	4,909	5,290

Notes: The dependent variable is the child's earnings score measured in 1992 or 2004. The independent variable in the top panel is the total number or friendship connections. The independent variables in the bottom panel are the number of non-reciprocated out-degree, non-reciprocated in-degree, and reciprocated friendship connections. The estimates reported in the top and bottom panels come from two separate OLS regressions using the full set of controls. Child controls include gender, age, and age squared. Parental controls include gender, five personality traits, and IQ test scores. Parental outcomes include earnings score and years of schooling. Standard errors are in parentheses. ***, ** , and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

non-reciprocated received friendship claims (in-degree). The number of reciprocated friendships shows the smallest point estimates. Due to large standard errors, almost none of the coefficients is significantly different from zero. As for the composed measure, all effects are consistently larger when we use the earnings score in 1992 as the outcome.

Earnings Score of Friends (Quality of Network)

We next take another perspective on friendship ties and examine whether network quality, as proxied by the average earnings score of friends, has an impact on the child's outcome. As not all claims are observed, the sample size reduces by approximately two-thirds. Table 7 presents the estimation results for the average earnings score of friendship connections for the full sample and by gender of parent (in the same format as before).

The results in Table 7 are qualitatively similar to those reported in Table 5 for network size. If we do not control for child and parent characteristics, the average earnings score has a significantly positive impact on the earnings score of children, even though the network effect is moderate in size. A one-percentage-point increase in the average earnings score of friends raises the earnings score of children in 1992 and 2004 by approximately 0.04 and 0.07 percentage points, respectively. As before, the network estimates decrease considerably when we add child and parent

Table 7. *Marginal network-quality effects*

	(1)	(2)	(3)	(4)
Full sample				
Child earnings score 1992	0.040**	0.063***	0.047**	0.033
(N = 2,943)	(0.019)	(0.018)	(0.021)	(0.021)
Child earnings score 2004	0.070***	0.054***	0.026	0.006
(N = 3,189)	(0.019)	(0.019)	(0.021)	(0.021)
Mothers				
Child earnings score1992	0.094***	0.094***	0.076***	0.071**
(N = 1,602)	(0.030)	(0.029)	(0.029)	(0.029)
Child earnings score 2004	0.086***	0.077**	0.048	0.038
(N = 1,709)	(0.031)	(0.030)	(0.030)	(0.030)
Fathers				
Child earnings score 1992	0.018	0.041	0.018	−0.009
(N = 1,341)	(0.031)	(0.030)	(0.030)	(0.030)
Child earnings score 2004	0.054*	0.041	0.006	−0.030
(N = 1,480)	(0.030)	(0.030)	(0.030)	(0.030)
Child characteristics		✓	✓	✓
Parent characteristics			✓	✓
Parent outcomes				✓

Notes: The dependent variable is the child's earnings score measured in 1992 or 2004. The independent variable is the average earnings score of friends measured in 1992. Each estimate involves OLS regressions with varying sets of controls. Child controls include gender, age, and age squared. Parental controls include gender, five personality traits, and IQ test scores. Parental outcomes include earnings score and years of schooling. Standard errors are in parentheses. ***, ** , and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

control variables. Here, the effect turns insignificant for both earnings scores. If we look again at the network effects for mothers and fathers separately, we observe similar patterns as before although differences by gender of parent are less pronounced here. As maternal friends again show a stronger impact, the point estimate for the earnings score measured in 1992 remains statistically significant in the richest specification.

IV. Robustness Checks

Our regression results indicate that parental-friendship connections have little, if any, influence on the prospective earnings of children. This is by no means a trivial finding if friends of parents provide children with valuable information about job opportunities. We, therefore, perform additional robustness checks to see how sensitive our parental-network

estimates are to a number of potential threats: network endogeneity, network recall and measurement error, and network persistency.

Network Endogeneity

One natural concern is that the size and characteristics of friendship networks are endogenously determined. If there are unobserved factors that enable parents to form friendships and help their children to obtain better job qualifications, our network effects are biased and probably too high. However, in the absence parental-network effects, this appears less of a concern, unless parental-network effects are negative.[20]

To explore the role of these unobserved factors in more detail, we repeat the friendship analysis in the context of a friendship fixed-effects model. If high-school friends share some of the unobserved factors that are correlated with their social network and the earnings score of their children, the inclusion of friendship fixed effects in our regression models takes those unobserved factors into account. We construct pairs of parents with their first-claimed friend, and we assume that these pairs share the same fixed effect. In the empirical analysis, we thus identify the parental-network effect from the unshared high-school friends within the friendship pairs. Our friendship fixed-effects sample excludes (by construction) all parents without friends and contains 926 friendship pairs.[21]

Table 8, Panel B, reports the fixed-effects estimates for the total number of friends and the average earnings score of friends. Comparing these estimates with our baseline estimates, reported in Panel A, we find that most estimated network effects are smaller and even slightly negative. We also find that the fixed-effects estimates change less when we add other control variables. This is not surprising. If friends indeed share (some of) the confounding factors that might bias our network results, we should find that our fixed-effects estimates are insensitive to the inclusion of cognitive and non-cognitive skill measures. The only significant coefficients are measured for the impact of network quality on the earnings score of children in 2004, but the negative point estimates turn insignificant when we include the full set of controls. As the friendship fixed-effects network estimates continue to be small and mostly insignificant, we do not think that unobserved factors (shared by friends) mask the weak network effects we reported in the previous section.

[20] Two studies find negative, but modest, network effects (Bentolila *et al*., 2010; Pellizzari, 2010). In our context of parental-friendship networks, their argument would be that children are pressed, or advised, to choose occupations that are closer to those of their parents' friends but lesser fit to their own skills.

[21] Even though some claims are reciprocated, each friendship pair is included only once in the analysis.

Table 8. *Robustness checks*

Outcome	Network size				Network quality			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Baseline results								
Earnings score 1992	0.595***	0.571***	0.495**	0.417**	0.040**	0.063***	0.047**	0.033
(N = 4,909; 2,943)	(0.205)	(0.194)	(0.195)	(0.194)	(0.019)	(0.018)	(0.021)	(0.021)
Earnings score 2004	0.534***	0.536***	0.372*	0.214	0.070***	0.054***	0.026	0.006
(N = 5,290; 3,189)	(0.204)	(0.200)	(0.200)	(0.198)	(0.019)	(0.019)	(0.021)	(0.021)
B: Friendship fixed-effects results								
Earnings score 1992	−0.417	−0.215	−0.208	−0.217	0.035	0.001	0.003	0.096
(N = 1,446; 1,426)	(0.485)	(0.442)	(0.451)	(0.447)	(0.061)	(0.056)	(0.058)	(0.100)
Earnings score 2004	−0.015	−0.000	−0.097	−0.107	−0.170***	−0.191***	−0.159***	−0.122
(N = 1,562; 1,538)	(0.475)	(0.465)	(0.474)	(0.470)	(0.057)	(0.056)	(0.058)	(0.100)
C: Network based on sustained friendships								
Earnings score 1992	−0.043	−0.288	−0.263	−0.327				
(N = 1,254)	(0.458)	(0.432)	(0.440)	(0.437)				
Earnings score 2004	0.772*	0.709	0.635	0.546				
(N = 1,365)	(0.458)	(0.449)	(0.451)	(0.444)				
Child characteristics		✓	✓	✓		✓	✓	✓
Parent characteristics			✓	✓			✓	✓
Parent outcomes				✓				✓

Notes: The dependent variable is the child's earnings score measured in 1992 or 2004. In Columns 1–4, the independent variable is the number of friends. In Columns 5–8, the independent variable is the average earnings score of friends measured in 1992. Each estimate involves OLS regressions with varying sets of controls. Panel A reports baseline results. Panel B reports results of friendship fixed-effects estimation. Panel C reports results with the number of out-degree high-school friends reported in 2011 as the independent variable. Standard errors are in parentheses. ***, ** , and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 9. *Measurement error and marginal network-size effects*

VAR(U)	*VAR(FN*)*	*VAR(U)/VAR(FN)*	β	SE	β/SE(β)
0.0	1.86	0%	0.534	0.204	2.61
0.2	1.66	11%	0.599	0.216	2.77
0.4	1.46	22%	0.681	0.231	2.95
0.6	1.26	32%	0.788	0.248	3.18
0.8	1.06	43%	0.937	0.271	3.46
1.0	0.86	54%	1.154	0.300	3.84
1.2	0.66	65%	1.502	0.343	4.38
1.4	0.46	75%	2.150	0.410	5.24
1.6	0.26	86%	3.785	0.544	6.96
1.8	0.06	97%	15.788	1.111	14.21

Notes: The dependent variable is the child's earnings score measured in 2004. The independent variable is the number of friends. Results are reported for different noise-to-signal ratios. Column 1 reports the assumed variance of the measurement error $VAR(U)$. Column 2 reports the variance of the true number of friends $VAR(FN^*)$, which equals $VAR(FN) - VAR(U)$. Column 3 reports the noise-to-signal ratio. Columns 4–6 report corresponding network effects, together with standard errors and *t*-values.

Network Measurement Error

Another concern is measurement error in our network measures. We construct the measures based on information about high-school friends that is collected 18 years after high-school graduation. When parents make mistakes or have difficulties in recalling who their best friends are, there is measurement error in our network measure. If the measurement error is random, that is, unrelated to the true network measure, the estimated marginal effects are biased towards zero (classical measurement error). To test for the impact of this error, we treat the friendship network measure as a continuous variable and adjust the parameter estimates and standard errors by imposing predetermined noise-to-signal ratios in estimation.

Table 9 presents the marginal effects of the number of total friendship connections on the earnings score of children for different noise-to-signal ratios (which are reported in Column 3). The estimation results show only a modest increase in the true network effect for increasing degrees of measurement error ($VAR(U)$). Even if half of the observed variation can be explained by measurement error, the network estimate suggests that one additional friend increases the earnings score by only 1.154 percentage points, which is still small given an earnings-score standard deviation of around 20 percentage points. This simulation exercise shows that small estimates cannot be explained by classical measurement error in the friendship variables. Taking into account that the marginal effect further decreases when we control for parent covariates, the underlying error must be inconceivably high to obtain sizeable estimates.

Network Persistency

It is also unclear to what extent parents are still in contact with the high-school friends later in life. Although friends who kept in touch after high school are more likely to be reported, it is reasonable to assume that some of the claimed connections have not been maintained. As those friends are unlikely to affect the labor-market outcomes of each other's children, they will, by construction, lower the average impact of friendship connections. To address this concern, we rely on the most recent survey held under the WLS respondents. In 2011, the subsample of respondents who had at least one reciprocal friend in 1975 (complemented with a 15 percent random draw of other WLS respondents) were asked again to report up to three same-sex high-school friends they are still in contact with. This sample contains 1,558 observations. While the questionnaire does not explicitly refer to friendship claims in 1975, it provides an additional measure of out-degree network connections that allows us to draw inference on the importance of high-school connections later in life. Compared with the initial out-degree, the average number of friendship claims decreases from 2.25 to 1.42. About 40 percent of all the parents report to have the same number of friends in both waves. The correlation between the 1975 and 2011 out-degree equals 0.20.

Table 8, Panel C, shows whether sustained connections have a stronger impact on the earnings score of children. When we use the early earnings score of children as the outcome, we find negative point estimates for all specifications but none of them is statistically significant. On the contrary, the effect of sustained friendships on the earnings score in 2004 is larger compared with the initial out-degree and less sensitive to the inclusion of control variables. As not all high-school friendships have been maintained until 2011, it makes sense that the estimated network effect is larger among the long-lasting friends of parents. However, the estimates remain small and at most marginally significant, which confirms that high-school friends of parents have no substantial effect on the earnings score of children.

V. Conclusion

Motivated by the idea that children might incur labor-market benefits from their parents' social network, this study makes a first attempt to empirically test whether children are better off because their parents have a more elaborate social network. Using data on high-school connections of parents, we find evidence that children are slightly more likely to work in the same occupation as their parent's friends, but this association disappears once we take into account the similarity in occupational choices of children and

parents. When we analyze the network impact on the occupational earnings score of children (which quantifies the average payoff by occupations), we find that larger friendship networks of parents significantly increase the children's earnings score at the beginning of their working career, but the measured point estimate is very small. Furthermore, the impact disappears if we use the earnings score of children 12 years later as the outcome variable, which we consider a better proxy for lifetime earnings.

These findings together suggest that children do not work in occupations that pay higher wages because of their parents' friendship network. However, our findings are not the result of a well-defined natural experiment and must be interpreted with care. We can think of two possible interpretations. The first one is a selection interpretation; that is, children raised by parents with many high-school friends are different from children raised by parents with few high-school friends. This is consistent with the notion of biased network estimates in which omitted variables relevant to the occupational choice of children are negatively related to their parents' friendship network. We have little indication of what these variables might be. Our sensitivity analysis rules out a number of plausible candidate variables. This leaves us with the second interpretation, which takes our findings at face value; that is, children do not take advantage of their parents' friends. The recent findings of Kramarz and Nordström Skans (2014) using network data from Sweden support this view.

Appendix

We impute the expected number of received friendship claims based on the observed distribution and selection probability for each potential claim. As respondents can claim only same-sex friends, the imputation is performed separately for the network of female and male friends. Let p define the share of Wisconsin high-school graduates in 1957 who are not part of the WLS. Moreover, assume that the true in-degree for individual i is described by the variable in_i, which takes values $k = 0, 1, 2, 3, \ldots, n$. Then, the observed measure can be expressed as $\tilde{in}_i = in_i - b$, where b represents the number of in-degree connections we miss. b follows a binomial distribution with parameters in_i and p ($b \sim Binomial(in_i, p)$). To correct the in-degree, we first impute the distribution of in_i based on the distribution that can be observed for $\tilde{in}_i$. Denote the observed share of $k = 0, 1, 2, 3, \ldots, n$ claims as $\tilde{q_k}$ and the underlying shares as q_k. Then, the observed shares $\tilde{q_k}$ are predicted by the true shares: $\sum_{l \geqslant k}^{n} \binom{l}{k} q_l (1-p)^k p^{l-k}$. To estimate q_k, we minimize the squared difference between observed shares and their predictions subject to the constraints that the underlying q values sum to one and are bounded between zero and one:

$$\min_{\{q_0,\ldots,q_n\}} \sum_{k=0}^{n} \left[\tilde{q_k} - \sum_{l \geqslant k}^{n} \binom{l}{k} q_l (1-p)^k p^{l-k} \right]^2, \tag{A1}$$

$$\text{s.t.} \sum_{l=0}^{n} q_l = 1 \; and \; 0 \leqslant q_k \leqslant 1 \forall k. \tag{A2}$$

As friendship information is available from 9,138 respondents out of approximately $3 \times 10,317$ high-school graduates in 1957, the probability that a graduate is not observed amounts to $p = 1 - 9,138/(3 \times 10,317) \approx 0.705$.[22] The potential number of received claims (N) can theoretically be as large as the whole population minus one. Given that we observe only up to six received claims (i.e., $\tilde{q}_k = 0 \; \forall \; k > 6$), the optimization becomes less precise if many (or all) potential q values need to be estimated. Therefore, we assume that the maximum number of potential friends is 43, which corresponds to approximately 25 percent of the average size of a school cohort in the WLS. As the probability of having more than 43 friends is very close to zero, imposing this restriction barely affects our results. Finally, the imputed shares $\{\hat{q}_0, \ldots, \hat{q}_{43}\}$ are used to calculate the expected in-degree of each respondent based on the observed number of received claims k:

$$\hat{in}_i(k) = \sum_{l \geqslant k}^{43} \binom{l}{k} l \hat{q}_l (1-p)^k p^{l-k}. \tag{A3}$$

For respondents who claim friends that are not covered by the WLS sample, the number of reciprocated friends is also measured with non-classical measurement error. Therefore, we impute the expected number of reciprocated connections exploiting the fact that friendship ties conditional on the number of claims are missing at random. Again, expected values are calculated separately for female and male friends. The dynamic imputation procedure consists of five steps and relies solely on information about observed reciprocal behavior.

First, respondents are sorted according to the number of claimed friends ($s_i = 0, 1, 2, 3$). Next, we calculate the respective average number of reciprocal friends ($\bar{r}_s$) for the subset of respondents with all connections in the sample. This information is used to impute expected reciprocated friendships ($\hat{r}_{s,i}$) for individuals with one missing claim. After using the imputed values to update the averages $\bar{r}_s$, we estimate the expected number

[22] We have to assume that non-response is uncorrelated with the number of friendship connections.

for respondents with two missing claims. Finally, $\bar{r}_s$ is updated again and used to impute values in the case that all three claims are not observed.

The imputation procedure could be further extended by additionally considering observable characteristics (see Conti *et al*., 2013), or the order of claims. However, a further differentiation between friendship ties would lead to less accurate estimates because they are based on fewer observations.

Supporting Information

The following supporting information can be found in the online version of this article at the publisher's web site.

Online Appendix

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